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The central purpose of research and development is to exorcise uncertainty. Because exorcism of any sort is a most risky undertaking, it clearly is advantageous to limit the quantity of uncertainty that must be dealt with at the onset. Not only is the scope of the R&D task thereby compressed, and its apparent difficulty lessened, but the probability of success may be spectacularly enhanced. The temptation to exclude uncertainty by accepting as valid certain assumptions about the nature of the R&D process is irresistible; if those assumptions are valid the number of important variables and consequently the sum of the uncertainties may be appreciably decreased. Unhappily, the habit of assuming the validity of premises that have been little tested is also pronounced. That seems to be particularly true of military R&D, where it is not uncommon to see casual observations transformed into assumptions that take on the substance of doctrine and, in time, become specific rules of procedure that treat both the doctrine and its parent premises as hard fact. The observations may have been incomplete, or they may have been biased by the outcome of whatever event was being observed, or they may have been totally incorrect from the beginning. Analyses and evaluations that do not take account of these circumstances, of the basic shakiness of doctrine and

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its transitory nature, must of themselves be suspect. It seems appropriate to suggest, then, that a body of imperfect observation may have fathered doctrine that has become a mythography of military R&D.

The term mythography^{*} identifies a situation in which an unreal representation of events and their causation becomes widely acceptable and is eventually transcribed into a procedural ritual. Mythography certainly is not unique to military R&D, but its prevalence there may be sufficient to constrain meaningful evaluation of the military R&D process. The purpose of this paper is to suggest both the source and the extent of such constraints. Obviously, the topic has such breadth and depth that no more than a few observations and illustrations may be sketched in here; it is, however, a subject that may be of some interest to students and practitioners of military R&D, and it is perhaps not unbecoming to suggest that investigators have been nibbling at its edges for some years without having entirely appreciated its implications.^{**}

The problem may be illustrated in this way: measuring the effectiveness of any particular aspect of military R&D is difficult if transitory rules of procedure are treated as preordained or timeless truths, which seems too often the case. The difficulty is compounded if, as has sometime occurred, doctrine is the afterproduct of events imperfectly understood or erroneously interpreted. A cartographer working from limited knowledge can draw a map of a flat earth which satisfies him in all respects because it incorporates all that he knows from an imperfect observation of the world he sees. But a navigator working with compass and log may find it awkward to

^{*}Not a coined word, although one must range several dictionaries to assure himself of that.

^{**}See, for example, Carl Kaysen, "Improving the Efficiency of Military Research and Development," in Public Policy XII (Harvard University Graduate School of Public Administration, Cambridge, 1963), pp. 219-263, and the comments by P. W. Cherington and J. S. Dupré (pp. 274-301). Dupré's remarks concerning the validity of the "weapons system approach" (pp. 290-292) are particularly pertinent.

use, and if one begins with the premise that the earth is flat an evaluation of navigational efficiency based on flat-earth maps can lead to some most misleading conclusions.

Evaluations of military R&D in terms of product quality can be similarly compromised if one accepts as invariables certain conditions arising from ritualization of the R&D process -- a flat-earth map of a sphere, so to say. Among the factors often treated as invariables are (1) procedures inherited from earlier programs, which substitute for program flexibility; (2) a predetermined organizational arrangement, as an alternative for a structure shaped by the character of the undertaking; and (3) something that can best be described as a style of development, although it is in many respects a creature of the first two. Having had what appear to be excellent results with a given set of procedures or a particular form of organization, or a particular style of R&D, planners tend to prescribe their use in later R&D programs. Similarly, program managers tend to derive general rules from observations of what appear to be obvious and readily explainable phenomena. Over time, such procedures or rules of thumb are written out, in rich detail, in thick sets of directives and regulations.

Examples are readily come by. In the early 1950s the weapons systems approach was composed, arising in conceptions of what were assumed to be grave shortcomings in the earlier evolutionary process of weapons development. (Whether the earlier process was indeed evolutionary in its character or whether the shortcomings complained of were those actually at fault seems not to have been too closely examined.) The concurrency thesis was a product of ballistic missile experience. Recently, a practice of elaborate pre-contract program definition has been conceived, refined, and reduced to specific directives for general adoption. It had its origin, in part, in the presumed limitations of the weapons system approach as it had operated in the late 1950s. The organization of the military R&D establishments was, in each instance, overhauled to correspond to the premises of the newly adopted doctrine and subsequent development was conducted accordingly.

Accompanying this elaboration of doctrine was a steady growth in the size of the average military project office and in the size of the average development team. That growth was certainly the product of a policy that favored considerably more detailed supervision of weapons development than had previously been customary, and the new doctrines clearly involved increasingly close governmental control of ongoing military R&D. Reporting requirements were more elaborate, review more frequent and more comprehensive, and in general the management of individual weapons developments steadily became more complex. In order to supply the detailed information required by various program directives, all of which were presumably essential to development management, R&D contractors have been obliged to enlarge their own management and engineering groups. Both the growth in engineering investment and the increasing involvement of the management process are usually credited to the mounting complexity of weapons themselves, although that assumption ignores some striking examples of technically complex weapons successfully developed by relatively small project groups exempted from the usual ritual. There is a growing suspicion that a style of development based on elaborate procedures and organizational arrangements may be more directly responsible for the increased cost and uncertainty of R&D than anything as seemingly tangible as increasing technical complexity. The proposition does not readily lend itself to analysis, but it may well deserve the attention of R&D planners.

The object of development regularization is nominally to eliminate as much uncertainty of cost, schedule, and technology as is possible. Present custom is to substitute for prospectively high-cost, high-risk tasks that make schedules and costs most uncertain, other tasks involving less difficult technology, with a consequent reduction in the anticipated uncertainty of estimates. The premise is that technological risk is the dominant element in program uncertainty, although there are some indications that other factors may deserve to be given at least as much weight. In any case, if a low risk approach means slower progress, and if it produces fewer or less

remarkable or less frequent technical advances, it is customary to take comfort in the assumption that any stoginess can be purged in time of need by program acceleration, which essentially means subordinating cost and technical assurance considerations to other factors, chiefly urgency. Although the practice of extremely tight but inherently realistic scheduling that marks most of today's military R&D may have made program acceleration an unrealistic option, it remains the classic assumption at the end of this chain of premises. In any discussion of R&D strategy one is certain to encounter the suggestion that ongoing R&D can be accelerated if one is willing to pay the price.* Again, that is a difficult proposition to test, but some evidence can be gathered.

Comparing the rates of progress of what may be called normal programs with the rates of accelerated programs offers some insight into the validity of the acceleration thesis. Figure 1 shows a plot of the key milestones in the development of five "normal programs" conducted by the Air Force in the late 1950s. Each was rated successful in terms of the criteria in use at that time.** The average program took a fraction more than five years to proceed from initial proposal to first squadron delivery. The system that made that transition in the least time needed almost four years and the system that required the most time, seven years.

For purposes of comparison, milestones have been plotted for a second set of Air Force system developments, in this case programs which for one reason or another were accelerated after they had begun routinely (Figure 2). Like those earlier illustrated, each was also considered "successful" in its outcome. For the accelerated

*See particularly M. J. Peck and F. M. Scherer, The Weapons Acquisition Process: An Economic Analysis (Harvard University, Boston, 1962), pp. 257-260, 263-264 and 285-286.

**The "knee" in line A which suggests that the average is better than the best case is caused by the inclusion of one sample program marked by a very rapid transition from initial proposal to contract.

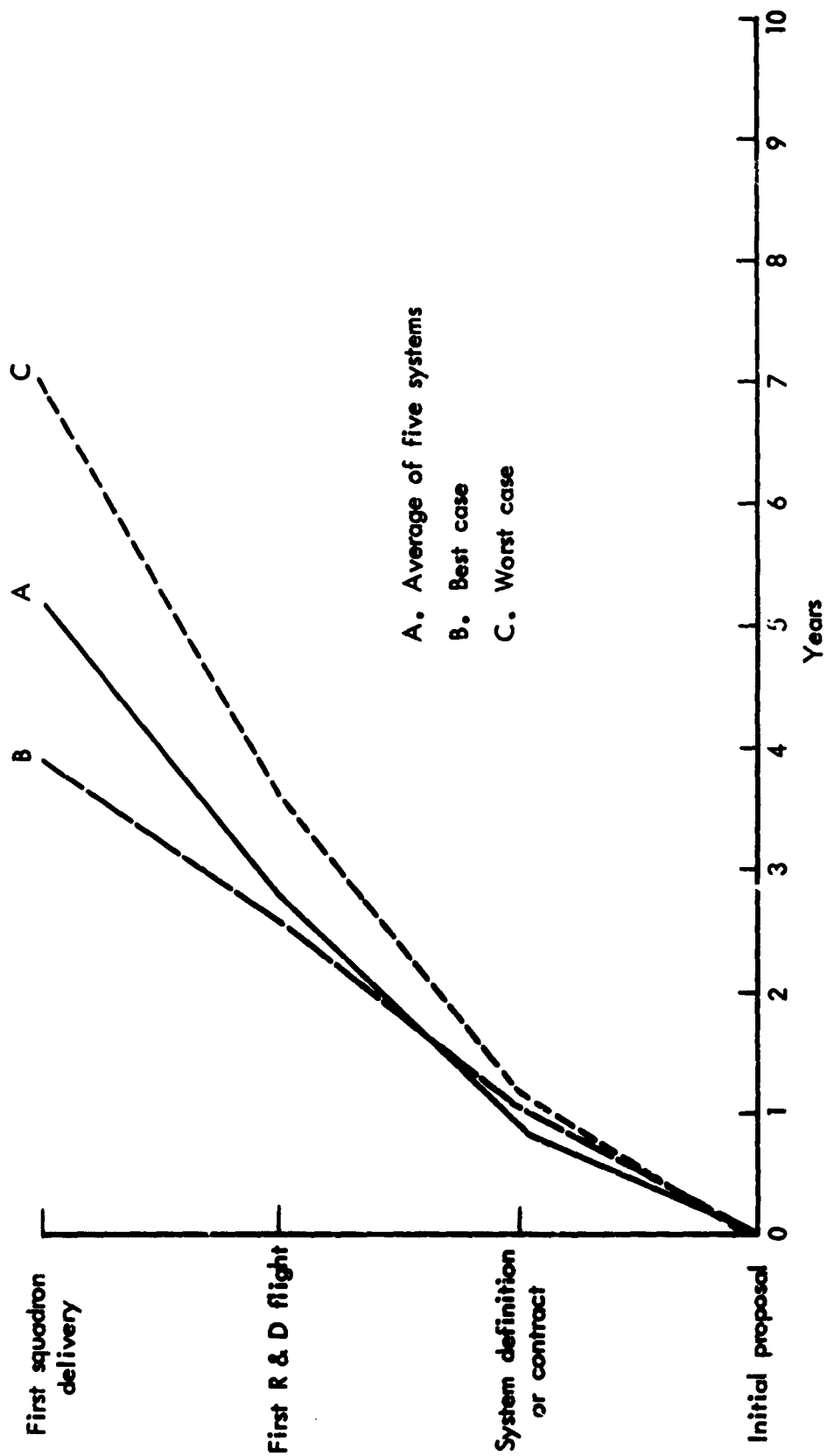


Fig. 1—Time required for five "normal development" aircraft programs

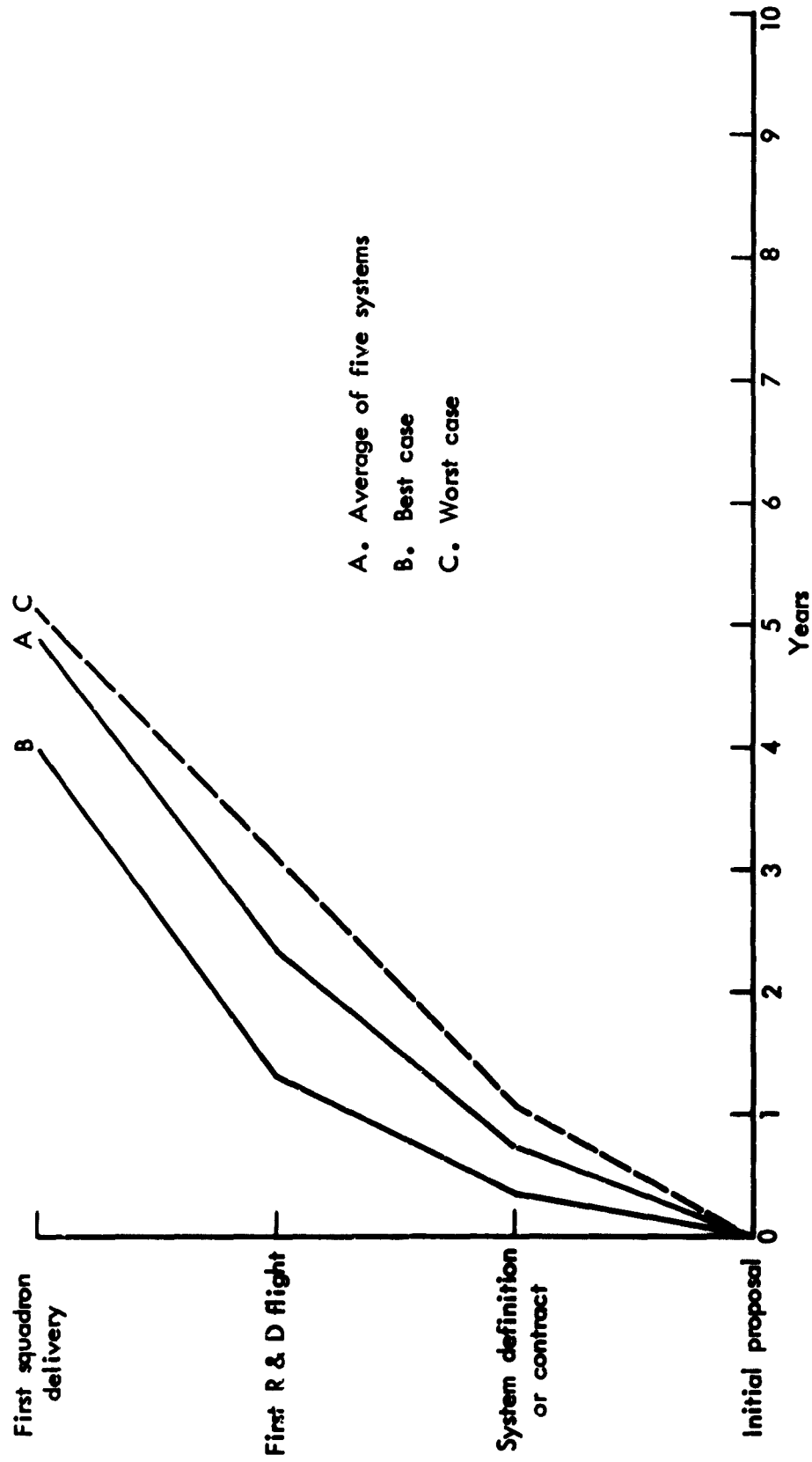


Fig.2—Time required for five "accelerated development" programs

programs a fraction less than five years was required to go from initial proposal to first squadron delivery. The best case needed a bit more than four years, and the worst case needed about five and one-quarter years. Differences among the programs are greater than the differences between the averages of "normal" and "accelerated" programs, which is at least interesting though perhaps not significant.

Some intriguing sub-aspects to these plots also deserve notice. For example, the time needed to pass from system definition or its equivalent to first flight was remarkably consistent for all systems, regardless of their category. In the case of accelerated systems, the departure from the norm was only .5 years; for normal programs, .75 years. In the progression from first flight to first operational availability, the average for normal development works out to 2.5 years, the best case to 1.95 years, and the worst case to 3.1 years. This represents a variation about the average of approximately .6 years. For accelerated developments, the average again is 2.5 years, but here the best case is 1.5 years and the worst case 3.2 years. The variation then is roughly .85 years. Under the most forgiving sorts of ground rules, a variation of six or seven months in the time required to proceed from first flight to first operational availability cannot be considered significant. Nor is it significant that the "normal" programs scored fractionally higher in this regard than the "accelerated" programs.

No extreme conclusions should be based on having looked at this small sample, but at the least the results indicate that program acceleration, as it is commonly conceived of, may not be a certain way of enhancing the early availability of new systems, assumptions to the contrary notwithstanding.

One of the most persistent assumptions in military R&D doctrine is that the careful preplanning and tight scheduling of complete system developments and the early integration of subsystems results in the earliest availability of fully capable equipment. A concomitant proposition is that an intensively managed concurrent

development can markedly accelerate the availability of fully developed systems. It is also an article of faith that concurrency is a costly way to go at development. Chiefly on the basis of what has been observed in the ballistic missile programs, concurrency is assumed to be an attractive development strategy only for most urgent programs in which cost is a minor consideration. But it is not at all clear that the central assumptions about concurrency are supported by pertinent evidence. It may be that concurrency is a costly way to develop systems, or it may be that missiles are cost systems to develop, regardless of technique. Although concurrency may be a way of assuring the simultaneous availability of critical subsystems, it is also possible that the availability of critical subsystems for the ballistic missiles was insured by other means -- reliance on parallel development, for example. The evidence is not clear. R. R. Nelson has examined the assumptions about parallel development, its rationale and its real costs, and has demonstrated that some of the "obvious" reasons for avoiding parallel development are entirely irrelevant. Under given circumstances it can be both more certain and less costly than single-course development.* Concurrency has been subjected to no similar scrutiny, but it is no more than reasonable to suggest that much that is said of it, or assumed, may also be of dubious worth.

One minor test of the concurrency thesis can be attempted. The relationship between engines of two sorts and the vehicles in which they ultimately were installed is an interesting illustration. Figure 3 shows a plot of the key milestones in the development of five turbojet engines and, overlaid, a comparable plot of four key milestones in the development of the aircraft systems in which they ultimately were installed. Partly as a demonstration that the relationship is not unique to aircraft, Figure 4 shows a counterpart

* R. R. Nelson, The Economics of Parallel R and D Efforts: A Sequential-Decision Analysis (The RAND Corporation, RM-2482, November 1959).

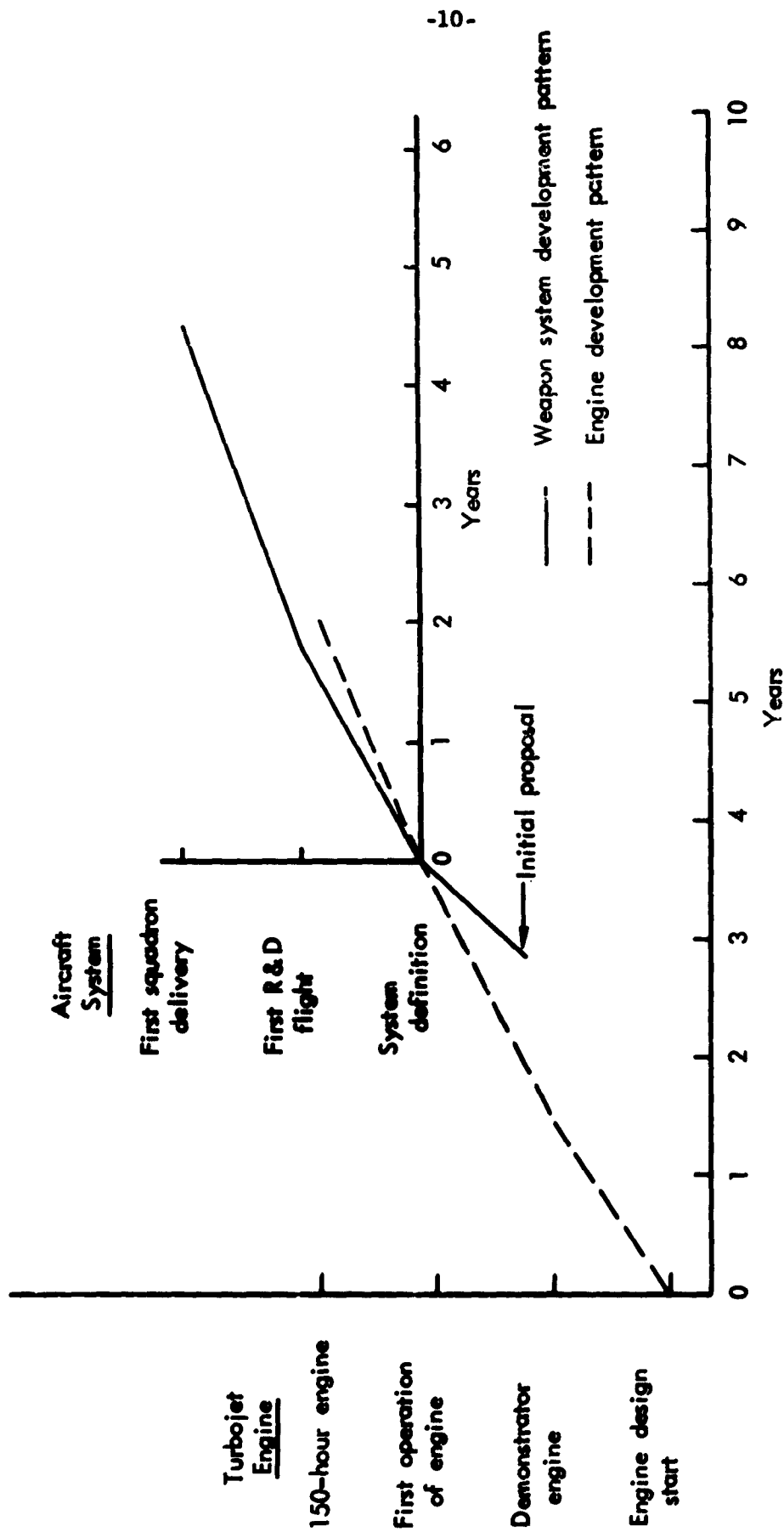


Fig. 3—Weapon system development time — average system development pattern (five aircraft)
overlaid on average engine development pattern (three turbojet engines)

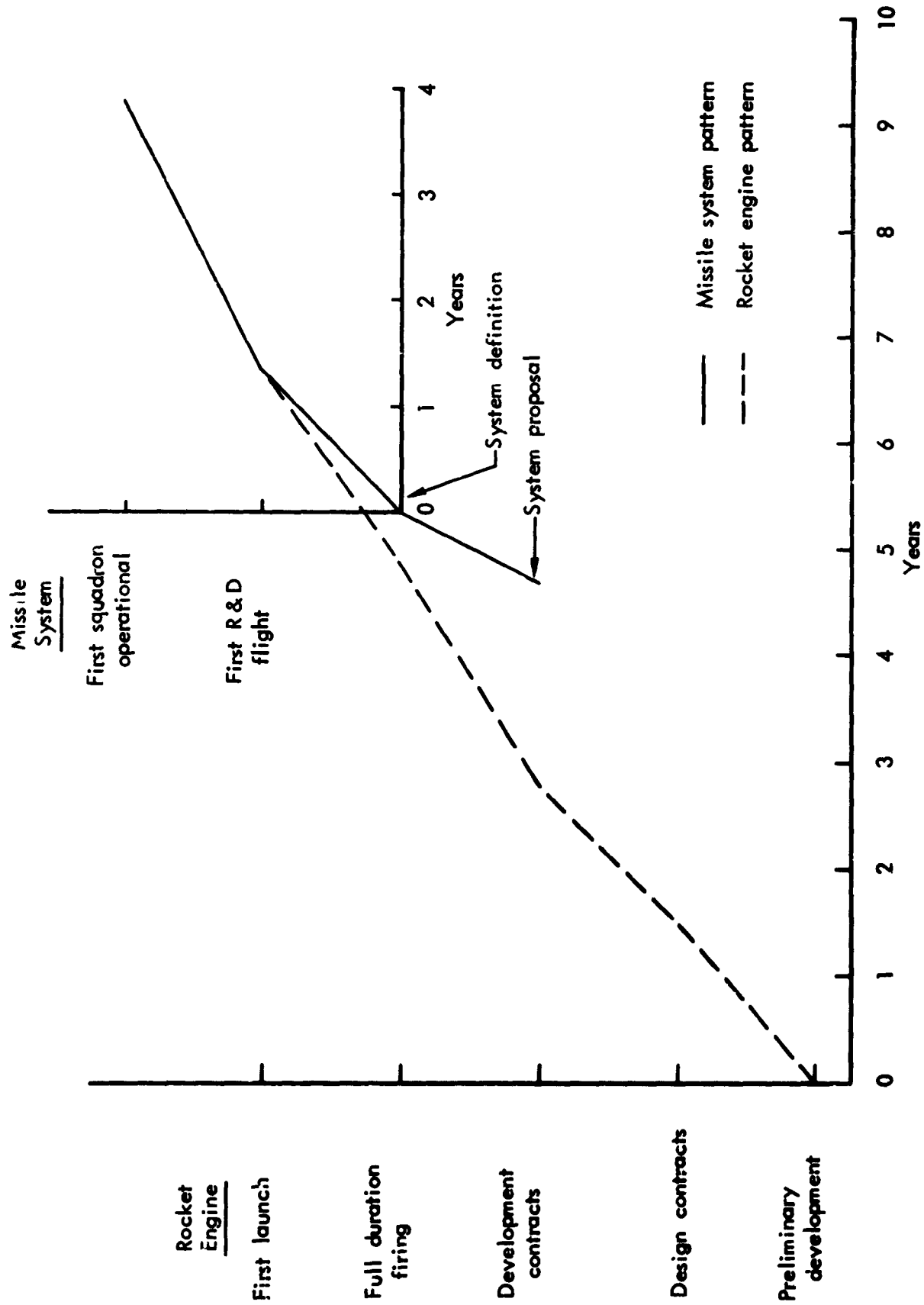


Fig. 4—Weapon system development time — average system development pattern (five missiles)
overlaid on average engine development pattern (three rocket engines)

plot of the relationship between three rocket engines and the missile systems in which they were eventually used. Although each of the five aircraft was designed as a system, that is, with all subsystems specified at the time development began, only one went to an operational squadron equipped with the engine originally scheduled for it and in that case the engine had been tested before the aircraft design was laid down. On the average, the engine finally used in the operational aircraft was programmed for that aircraft more than three and one-half years after engine development had begun. To put it another way, not until an engine had been in development for more than three years and not until a demonstrator had actually been operated was it ordinarily possible to program a given engine for a given application with any assurance that the assignment would prove valid. In the case of the missile programs here illustrated, at least three years of preliminary engine development had occurred before it was possible to plan with assurance for a combination of propulsion system with the balance of the missile system. By reputation, each of the missile systems involved concurrent development; concurrency, as it would today be defined, was also a characteristic of four among the five aircraft systems treated here, but in each instance the engines being "developed concurrent with" the airframes proved inadequate in one respect or another and substitutes were used.

This example alone may not be sufficient to raise major questions about the validity of the concurrency thesis, but it does at least suggest the possibility that concurrency, in the sense of its present usage, may not have actually been used in the missile programs and may not have been successful in the aircraft programs. In that case, the costs charged to concurrency may not be so chargeable at all. Or an explanation may lie in the proposition that engine development time is inherently incompressible (allowing for differences between programs), which would suggest that a system development should not be attempted under any circumstances until the propulsion system has been adequately demonstrated.* There are some other obvious

* See T. A. Marschak, The Role of Project Histories in the Study of R & D (The RAND Corporation, P-2850, January 1965), Ch. III.

possibilities, of course, but the main point is that too little is known about the process of development to encourage any great reliance on early predictions of system availability or the validity of theories of concurrent development.

If the engine example can be accepted as a valid representation of one crucial relationship between system and sub-system, which seems reasonable enough, one of the vital keys to a successful development program would seem to be the assured availability of critical components. That certainly is not a novel observation,* although its acknowledgement and one recent consequence -- pre-approval program definition -- have caused a surprising amount of comment. But it also appears that an undue emphasis on the fact alone, without a counterpart effort to develop readily identifiable non-critical components well in advance of the start of a system development, could lead to a new lot of doctrinal rules as shaky as many of the earlier prescriptions.

Some of the other dangers of reading too much into experience and of applying perceived lessons too broadly may be appreciated from a review of the Air Force development of first-generation ballistic missiles. Two factors, uniquely efficient management techniques and the application of concurrency to development -- by which is meant bringing to simultaneous fruition a diversity of difficult technical tasks -- have been given the largest credit for that achievement. As has earlier been observed, one should approach the concept of development concurrency with due caution. Its liturgy is sprinkled with incantations and rote phrases. The term "management of technology" appears to be a way of saying "common sense procedures." Communication, reporting, and decision procedures, for example, were notable for their Spartan character. Internally and externally, the Air Force ballistic missile organization of the late 1950s was

*See Peck and Schever, pp. 37-38; B. H. Klein, "The Decision Making Problem in Development," in The Rate and Direction of Inventive Activity (Princeton University, Princeton, 1962), pp. 478-479.

marked by short and direct lines between technical people and senior project directors. Procedures were relatively informal, often originating in response to an immediate and transitory need without much regard for protocol or tradition. That situation was in marked contrast to the norm of military research and development, in which both the structure of the organization and the operating procedures were predated, making slight allowance for the immediate character or intrinsic needs of the programs themselves. One is entitled to ask whether "concurrency," or "management of technology" had as much to do with the ballistic missile achievement as did classic virtues like communication efficiency or program informality. Here is a case in which the obvious may have been overlooked because the novel was more attractive.

Finally there is the matter of program style, which may well have been the most important distinguishing characteristic of the ballistic missile program, but which is extremely difficult to quantify. It is no great chore to set down, after the fact, a resumé of those organizational and procedural patterns that in retrospect appear to have contributed most to the success of a given program. The temptation to emphasize uniqueness is overpowering; the main difficulty is in distinguishing between the apparent and the real, the novel and the repainted mundane. In the case of the early ballistic missiles, for example, test results were very quickly fed back into the technical program and the development test missiles were altered accordingly. Improvements in the general level of missile adequacy were, therefore, rather quickly transferred to production missiles. But the very rapidity of the process in combination with an unquestioned urgency of production resulted in the deployment of several models and sub models of each missile, and they tended to have an indifferent resemblance to one another. The remedy was -- somewhat after the fact -- to devise a rather elaborate configuration control mechanism and impose it on later programs.

It is reasonable to ask whether the remedy was entirely appropriate to the disorder, or for that matter, if the real disorder was

ever properly identified. The advantage of rapid technical feedback lay in prompt and continuing improvement of the development system, in what might properly be characterized as a classically effective R&D process. That the process was expensive, as operated, and that it caused the deployment of several versions of a basic missile, may or may not be valid generalizations. To ask "more expensive than what?" and "is a standard configuration really vital for strategic missiles?" seems little enough. Configuration diversity probably stemmed more directly from the urgency of production than from the nature of the development procedure itself. Yet the outcome, costly production of model variants, has generally been taken to be an invariable of what is called concurrency, although it may indeed have no special relevance to that doctrine -- which, as earlier remarked, may be suspect on other grounds too.

A related matter that requires attention derives from a casual reliance on data derived from partial records of the past. Many of the projections of cost and time that characterize R&D planning are based on extrapolations of recent experience. That is, if Programs A, B, and D have cost so much and taken so long, proposed Program C, generally comparable in scope, is valued at about the same levels. In any lot of similar programs using common procedures and a common R&D doctrine it is unlikely that remarkable variances from normalized costs and schedules will occur. But the assumption remains valid only if procedures and doctrine are static. Can one ignore the possibility that given procedures may be responsible for the apparent stability of costs? There is reason to question a proposition based on invariables that are treated as invariable largely because changing them would inconveniently upset the equation and probably invalidate the estimates.

Those concerned with improving the effectiveness of military research and development might well think of beginning by abandoning reverence for a great many preconceptions on how research and development could be or should be or has been effectively conducted, no matter how well founded in theory or retrospect those conceptions

appear to be. A considerable fund of information on origins and sources, on events and their causes, must be gathered and appraised before we can put faith in many of the assumptions that are accepted as invariables of the military R&D process. Prescriptive doctrine and procedure are particularly suspect. Each program, or problem, deserves to be considered on its own merits and in terms of its own environment. One must take special pains to insure that the factors he is evaluating were indeed influential in a given program or class of programs. Nor may we safely ignore the possibility that the effectiveness of military R&D in a given case has been influenced by differences in what are ordinarily taken to be the invariables, and that the obvious or generally credited influences are rather less influential than may be casually assumed. However great the temptation to think otherwise, it seems best to treat military research and development as an area in which myth, legend, preconception, and misinformation are abundant; where one often has no alternative to being more qualitative than quantitative; and where distinguishing between reality and illusion is no easy task.